A
CHEMICAL ACCOUNT
OF THE
BRIGHTON CHALYBEATE.

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FROM THE NEW EDITION OF
DR. SAUNDERS's "TREATISE ON MINERAL WATERS."

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A CHEMICAL ACCOUNT OF THE CHALYBEATE SPRING, NEAR BRIGHTON. *

THE Chalybeate Spring, near Brighton, commonly called *The Wick*, has long been noticed as a ferruginous water. But as far as I can learn, no regular account of its chemical or medicinal properties has ever been publiſhed. The only notice which I can trace reſpecting this ſpring, previous to the mention which Dr. Saunders has made of it in his work on Mineral Waters, is contained in the *Brighton Guide*, where a very curſory and imperfect account of the moſt obvious properties of this water is given, firſt on the authority of Dr. Relhan, whoſe obſervations are ſtated to have been made a conſiderable time ſince, and afterwards on that of Dr. Henderſon of Brighton. But theſe accounts, in the preſent ſtate of chemiſtry, can ſcarcely be of any other uſe, than that of enabling us to obſerve, that in earlier periods, this ſpring had not remained unnoticed.

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* The Chemical inquiry which is the ſubject of this eſſay, was ſuggeſted by Dr. Saunders, and undertaken in conſequence of his having expreſſed a wiſh that an opportunity might be afforded him to introduce, in the ſecond edition of his "*Treatiſe on Mineral Waters*," now in the preſs, a more particular account of the Brighton Chalybeate, which he had merely noticed in his firſt edition. This paper has ſwelled to a much greater ſize than was at firſt expected; but as it is not intended to be publiſhed in any other ſhape, Dr. Saunders, inſtead of giving only an abſtract of it in the above treatiſe, has done me the honour to inſert it at full length, allowing me to procure a few detached copies of it for my own uſe.

§ I. *Situation of the Spring.*

The ſpring iſſues from the declivity of a ſmall eminence, ſituated about the diſtance of half a mile to the weſtward of Brighton, and a quarter of a mile from the ſea. The aſcent from the ſea ſhore to the ſpring, is very gentle and inconſiderable; but the eminence from which it iſſues, is commanded on every other ſide, by a ſucceſſion of ſmall hills, which gradually riſe round it.

The hill which extends immediately to the weſtward of Brighton, and over which the Horſham road paſſes, conſiſts chiefly of lime-ſtone. Extenſive lime quarries are opened on the borders of this hill, and the ſoil for ſome diſtance continues calcareous. But on approaching towards the chalybeate ſpring, the ſoil gradually becomes argillaceous, and the particular ſpot on which it is ſituated, appears to conſiſt almoſt entirely of clay. Beſides clay, however, it was found, in clearing away the rubbiſh to form the reſervoir, that the ſoil in that ſpot, was intermixed with veins of a black oily combuſtible ſubſtance, ſome ſpecimens of which I have ſeen, which evidently contained a quantity of coaly and pyritic matter. * In the immediate vicinity of the ſpring, ſome fir trees and ſhrubs have been planted, which ſeem to thrive; but except the turf, and ſome ſcanty heath, no ſpontaneous vegetation is to be ſeen for a conſiderable diſtance. On the top of the eminence, and at a very little diſtance from the well, there is a pretty large pond, apparently ſupplied by a ſpring, but which has no particular taſte, or other ſtriking properties.

For the accommodation of thoſe who drink the water, a convenient building has been erected immediately over the ſpot from which

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* For theſe ſpecimens I was indebted to Dr. Tierney of Brighton. Mr. Tennant, to whom I ſhewed them, told me, that he had repeatedly found on the ſea ſhore, at Brighton, ſimilar fragments of coaly pyritic matter; a circumſtance which he thought then rather ſingular, but which ſeems now to be accounted for.

the ſpring iſſues, where the water is received a few feet under ground, into a baſon of Portland ſtone. This reſervoir contains only a few gallons of water, but it fills again as ſoon as it is emptied, and is prevented from overflowing by a drain, which conducts the ſuperfluous water into a contiguous pond. This baſon, I obſerved, has its internal ſurface deeply corroded by the water, and its bottom is covered with a thick yellowiſh ſediment, which ſhews itſelf abundantly wherever the water is allowed to ſtagnate, and particularly in the ſmall crevices formed by the decayed brick-work which ſurrounds the well.

§ II. *More obvious qualities of the water.*

(A)—On firſt inſpecting the baſon, early in the morning, and before the water has been ſtirred, its ſurface is commonly found covered, ſometimes entirely, ſometimes only partially, with a very thin irideſcent pellicle; and beſides this, when the water has not been diſturbed for ſome hours, there is alſo often a kind of yellowiſh ſcum, floating in irregular patches on its ſurface. The ſpring is at all ſeaſons very plentiful, and does not appear to vary ſenſibly in its qualities. Yet after heavy rains the water is ſometimes ſlightly turbid. The temperature of the well was found to be 54°, when the thermometer ſtood in the air at 68°. I have been told by people who live on the ſpot, that the ſpring has never been known to freeze.

(B)—The water, after the ſmall quantity of ſcum juſt mentioned has been removed, is quite clear and tranſparent, and no gas is ſeen to eſcape from it, although, if poured high from one veſſel into another, there is a ſort of ſparkling appearance, which, I believe, would be found to be common to all waters, and ought not to be miſtaken for a diſengagement of air bubbles.

(C)—The Wick water, when quite freſh, has a peculiar faint ſmell, not uncommon in ferruginous waters, and a ſtrong, though not unpleaſant, chalybeate taſte. Its ſpecific gravity is 1001.08.* It inſtantly curdles ſoap. Some of the water which had been allowed to ſtand a whole night in a glaſs tumbler by the ſide of the well, with ſome of the ſcum above deſcribed floating on its ſurface, was found the next morning ſtill perfectly clear; but the greateſt part of the ſcum had precipitated to the bottom of the glaſs, in the form of a yellowiſh ochry ſediment, in which I could only perceive a kind of faint earthy taſte.

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* The ſpecific gravity was taken twice with ſufficient care and accuracy, and with ſimilar reſults. But in both caſes the water was three or four days old, before I could have an opportunity of aſcertaining its denſity.

(D)—Having taken to my lodgings, in a glaſs bottle, a gallon of this water, free from any ſcum or ſediment, it continued perfectly clear the next day, and exhibited no appearance of precipitation or any change whatever, except perhaps that the peculiar ſmell, which I have mentioned, was leſs perceptible. On the third day, ſome ſmall air bubbles appeared on the ſurface of the water, but I could perceive no other change. The day after however, on removing the ſtopper, I remarked a ſlight hiſſing noiſe, as if a quantity of air was making its eſcape. The bottle had been kept well ſtopped, but as it was not full, a quantity of atmoſpheric air had remained in contact with the water. At the expiration of five or ſix days, the water was found ſomewhat turbid, and ſmall yellowiſh particles were ſeen floating in it. It had loſt entirely its peculiar ſmell, and the chalybeate inky taſte was leſs perceptible. After a few weeks, a conſiderable yellow ſediment had ſubſided, the water had become more turbid, and the chalybeate taſte was farther diminiſhed. Some of the ſame water, which had been carefully corked up in a tranſparent glaſs veſſel (no air being left in contact with it, and the veſſel remaining at complete reſt), preſerved its tranſparency much longer. Yet after a few weeks, this water alſo was found to have depoſited a yellow ſubſtance all over the inſide of the veſſel. But the water itſelf, on being poured out, after ſtanding for near two months, and when the chalybeate taſte was conſiderably diminiſhed, appeared quite clear, leaving behind the yellow ſediment, which adhered ſtrongly to the ſides of the bottle.

§ III. *General effects produced on the water by chemical teſts.*

Having now ſtated the ſpontaneous changes which this water undergoes, and its moſt obvious properties, I proceed to relate the general reſults obtained by the application of teſts, or chemical re-agents.

EXPERIMENT I.—Some of the water, juſt brought from the well, being heated in an open veſſel, and kept boiling for a few minutes, no precipitation took place, and no other obſervable change was produced in the water, except a ſlight diminution of its tranſparency.

EXPER. II.—The ſame experiment being repeated in a tubulated receiver, the neck of which was immerſed in mercury, in order to prevent the acceſs of air, the water, after undergoing ebullition for a few minutes, appeared as tranſparent as before.

EXPER. III.—A ſmall quantity of the water being quickly reduced by boiling to about one-third of its volume, continued free from precipitate, although it did not appear quite ſo tranſparent. On attempting to puſh the concentration farther, a yellowiſh ſubſtance began to collect at the bottom of the veſſel.

Exper. IV.—Some of the water being ſlowly heated in an open veſſel, and kept for about an hour over a lamp, without being ever allowed to boil, a yellow precipitate began to take place, before the water had undergone any ſenſible concentration.

Exper. V.—The foregoing experiments being repeated on water that had been kept for ſome time, the ſeparation of yellow matter took place more quickly; and in general, there appeared to be ſome kind of proportion, between the readineſs with which the heated water yielded this precipitate, and the time which had elapſed after it was taken from the well.

Exper. VI.—Water, quite freſh from the well, altered paper ſtained with litmus, to a reddiſh purple colour.

EXPER. VII.—The ſame water, after being boiled, altered litmus exactly in the ſame manner; and the change of colour ſeemed even to take place ſomewhat more readily in the boiled than in the unboiled water.

EXPER. VIII.—Paper ſtained with turmeric had not its colour any way altered by the water, whether previouſly boiled or not.

EXPER. IX.—Paper ſtained with the red infuſion of Brazil wood, being moiſtened with the water, turned to a dark brown colour, with a faint purpliſh hue; and this effect took place, whether the water had been previouſly boiled or not.

EXPER. X.—Pruſſiat of potaſh inſtantly produced a blue cloudineſs, and after the ſeparation of the yellow particles mentioned in Exper. 3, 4, and 5, whether by boiling, or by keeping, ſtill the clear water continued to ſtrike blue with pruſſiat of potaſh, though evidently in a fainter way than before this operation.

EXPER. XI.—Tincture of galls being dropped into the water juſt brought from the well, produced, at firſt, neither cloud, nor change of colour. But on being allowed to ſtand, the mixture gradually became cloudy, and the next day it was found quite black and turbid.

Exper. XII.—Tincture of galls being mixed with ſome of the water, which had been kept for a week or two, the black precipitate took place immediately.

Exper. XIII.—The ſulphuric, nitric, and muriatic acids, produced no effervefcence, precipitation, or hepatic ſmell whatever. But on the contrary, if the water had begun to undergo the ſpontaneous precipitation before mentioned, any of theſe acids reſtored its pellucidity inſtantly.

Exper. XIV.—Oxalic acid produced no immediate precipitate; but a cloud appeared in the water after ſtanding for ſome time.

Exper. XV.—Both oxalat of ammonia and oxalat of potaſh, produced an immediate white precipitate.

Exper. XVI.—Cauſtic alkalis occaſioned a precipitation of yellowiſh flakes, and the ſame effect took place in water which had been boiled. Lime water produced ſimilar effects.

Exper. XVII. — Barytic water inſtantly produced a copious white precipitate, whether the water had been boiled or not; and this precipitate was not rediſſolved by adding muriatic acid.

Exper. XVIII.—Both muriat and nitrat of barytes, threw down a copious white precipitate.

Exper. XIX.—Nitrat of ſilver produced a whey-coloured cloud, which, on ſtanding, ſubſided, and paſſed to a grey colour.

Exper. XX.—Having previouſly added a few drops of nitric acid, and of nitrat of barytes, till no further precipitation took place, ſtill the water gave a copious precipitation with nitrat of ſilver.

Exper. XXI.—Nitrat of mercury occaſioned a white precipitate, which continued white after ſtanding for ſome days.

Exper. XXII.—Nitrat of ſtrontites produced no precipitate at firſt; but after ſtanding for a day or two, a white incruſtation was formed on the ſides of the glaſs.

Exper. XXIII.—Acetite of lead only produced at firſt a ſlight cloudineſs; but after ſtanding for a day or two, a pretty copious purpliſh powder ſubſided.

§ IV. *Inferences drawn from the preceding experiments.*

From the preceding experiments ſome inferences were deduced, reſpecting the particular ingredients contained in this mineral water, which prepared the way for a more minute inveſtigation. It was ſoon perceived, that in ſeveral of the above experiments, uſeleſs redundancies had occurred; and if I have not ſuppreſſed them here, it is becauſe I wiſhed to relate faithfully the gradual progreſs of this inquiry, and the various methods which I have uſed in proſecuting it.

1ſt, From experiment 1, 2, and 3, it appeared obvious, that no part of the ſolid ingredients of this water was kept in ſolution by means of a gaſeous acid; ſince boiling, even to a conſiderable extent, did not produce any precipitation.

2dly, From experiments 2, 3, 4, 5, it was inferred, that the acceſs of air promoted in this water, the precipitation of ſome metallic ſubſtance.

3dly, From experiment 6, it was ſuſpected that the water was ſlightly acid; but, from experiment 7, it appeared evident that this circumſtance did not depend upon the preſence of an aerial acid.

4thly, It was inferred from experiment 8, that neither pure nor carbonated alkali, nor pure earth were preſent in the water. From experiment 9, a ſmall quantity of carbonated earth might have been ſuſpected, but ſuch an inference being incompatible with experiment 13, the ſlight change of colour alluded to, in the infuſion of turmeric, was ſuppoſed to depend on ſome other cauſe.

5thly, The preſence of iron was clearly indicated by experiment 10; and it was conjectured from experiment 12, that the ſlowneſs with which tincture of galls had produced its effect in experiment 11, was owing to a want of ſufficient oxygenation. The circumſtance of a quantity of oxyd of iron being precipitated from the water by a gentle heat (exp. 4) whilſt on the contrary, no precipitation took place (exper. 3), when the water was boiled briſkly, could only be accounted for by the action of the atmoſphere, which, in the firſt inſtance, oxygenated the iron, whilſt in the latter, its acceſs was prevented by the rapid emiſſion of ſteam.

6thly, Both experiments 14 and 15, but more eſpecially the latter, indicated the preſence of lime; but from ſome of the reſults above mentioned, it was obvious, that the lime, in this inſtance, was combined with a mineral acid.

7thly, The experiment 16, confirmed the former concluſions with regard to the exiſtence of iron in the water; but did not afford ſatisfactory information as to the exiſtence of earths, as theſe might eaſily be confounded with the oxyd of iron.

8thly, The experiments 17 and 18, indicated the preſence of ſulphuric acid, and it became probable that the alteration of litmus, obſerved in experiment 8, depended either upon this acid, in a ſeparate ſtate, or upon ſome of its compounds, which have the ſame property with regard to blue vegetable colours.

9thly, From experiments 19 and 20, it appeared extremely probable, that the water contained muriatic acid.

10thly, From the colour of the precipitate obtained in experiment 21; from the effect of the mineral acids (experiment 13); and the total abſence of hepatic ſmell, it appeared ſufficiently obvious, that the water was free from hepatic gas, or hepatic compounds.

§ V. *Plan of Analyſis.*

On collecting the information derived from theſe general reſults, it appeared that the only ſubſtances which I could poſitively expect to find in the water, by a more particular inveſtigation, were iron, combined with the ſulphuric, or perhaps with the muriatic acid, and lime, in the ſtate of ſulphat, or poſſibly in that of muriat. I ſuſpected the preſence of alum, Epſom, and common ſalt; but this I did from conjecture alone, and upon no other grounds, than a compariſon of this water with other mineral ſprings of an analogous compoſition. With regard to the gaſeous contents, the only ſubſtances of this claſs, which I could reaſonably expect to find, were atmoſpheric air, and carbonic acid gas; but neither the one nor the other had yet been poſitively ſhewn, and on the contrary, ſome circumſtances had occurred, which rendered the preſence of the latter rather improbable.

Theſe general notions however, enabled me to form the following plan of analyſis;

1ſt, To examine the *gaſeous contents* of the water.

2dly, To obtain by evaporation, the *fixed ingredients* of the water in a ſolid form, as a previous ſtep to their chemical examination. For the ſtate of great dilution in which theſe ingredients appeared to exiſt in the water, and the great tendency ſhewn by ſome of them to be precipitated by concentration, would have made it exceedingly inexpedient to operate on the water itſelf.

3dly, To rediſſolve a known quantity of the reſidue obtained by this evaporation, and precipitate the iron from it, by pruſſiat of potaſh, or ſuccinat of ammonia.

4thly, To ſeparate the lime from the ſame ſolution by oxalat of ammonia.

5thly, To add cauſtic potaſh to the ſame ſolution, with a view to precipitate both the *magneſia* and *alumine*, if theſe earths ſhould exiſt in the water.

6thly, To boil this laſt precipitate (if any was obtained), in pure potaſh, in order to diſſolve the alumine, and thus obtain the *magneſia* in its ſeparate ſtate.

7thly, In caſe the potaſh ſhould appear to have taken up any *alumine*, to precipitate the latter by boiling the alkaline ſolution with muriat of ammonia.

8thly, To diſſolve another portion of the reſidue in nitric acid, and to add nitrat of barytes, with a view to aſcertain the quantity of *ſulphuric acid.*

9thly, To add nitrat of ſilver to the ſame ſolution, in order to precipitate the *muriatic acid.*

10thly, Laſtly, to try, by the agency of alcohol, which has the power of diſſolving certain ſalts, and of precipitating others; and by ſlow evaporation and cryſtallization, whether ſome light might not be thrown on other parts of the analyſis, and other ſubſtances diſcovered, which were not unlikely to exiſt in the water, but might have eſcaped notice, had the former method been excluſively employed.

§ VI. *Examination of the gaſeous contents.*

Exper. I.—A quantity of the water juſt brought from the well, and meaſuring exactly $5\frac{1}{4}$ cubic inches, was put into a phial, or ſmall receiver, the neck of which terminated in a bent tube, which had its extremity immerſed in a mercurial bath. The phial was not quite filled with the water, but the quantity of atmoſpheric air, contained both in the tube and the upper part of the phial, was accurately aſcertained. The heat of a lamp being now applied, an elaſtic fluid ſoon began to come over, which was collected in a graduated jar. After allowing the water to boil for about a quarter of an hour, no more gas was given out; and as the water itſelf was now beginning to boil over, the proceſs was ſtopped, and the reſult examined. For this purpoſe, the quantity of air collected in the jar, was carefully noted, and a ſolution of cauſtic alkali being introduced into it, a quantity of gas equal to $\frac{4}{10}$ths of an inch, was immediately abſorbed, which, of courſe, was carbonic acid.

Exper. II.—With regard to the portion of air left in the jar unabſorbed, it was found, (every allowance being made for preſſure and temperature), that its bulk was perfectly ſimilar to that of the air contained in the apparatus: and this air being examined by the nitrous teſt, gave preciſely the ſame reſult as atmoſpheric air. Theſe two experiments were tried a ſecond time with reſults exactly ſimilar.

Exper. III.—Some of the water which had been ſent from Brighton to London, and kept for five days, being tried in the ſame manner, yielded only the $\frac{17}{100}$th part of an inch, which is a little leſs than half the quantity obtained in the former experiment.

From theſe experiments it may be inferred that the Brighton chalybeate yields, when recently taken from the ſpring, about $\frac{1}{13}$th part of its bulk of carbonic acid gas, and that it does not appear to contain any other elaſtic fluid. From the readineſs with which this gas is given out; from the obvious diminution which it ſuffers by keeping; and from other circumſtances of the analyſis, it appears extremely probable that the gas exiſts in the water free from any combination.

§ VII. *Evaporation of the water.*

(A)—In order to prevent ſuch chemical changes as are liable to happen in operations of this kind, from too great a heat being applied, I was deſirous of carrying on this evaporation on a pretty large maſs of water at a very gentle heat. I had brought no apparatus with me for that purpoſe, but Mr. Glaſyer, a very intelligent and well-informed chemiſt and druggiſt, of Brighton, obligingly offered his aſſiſtance for this tedious operation. A whole gallon of water was carefully evaporated over a water bath, and the reſidue, which appeared in the form of a greeniſh maſs, was allowed to dry at the ſame temperature. This reſidue weighed exactly 55 grains, which makes for each pint $8\frac{1}{8}$th grains of ſolid contents, dried at 212°. It ſoon gave ſigns of deliqueſcence, and increaſed in weight by expoſure to the atmoſphere.

(B)—After this firſt trial, having often had occaſion, in London, during the courſe of this analyſis, to evaporate new quantities of water, which were ſent to me from Brighton, I found it much eaſier and more expeditious to begin by boiling the water down briſkly to a ſmall quantity over an Argand's lamp, in a glaſs veſſel containing only about one pint, and then to finiſh the evaporation and deſiccation over a water bath, at any deſired temperature. The heat which I have generally uſed for theſe deſiccations is that of 160°, and I have uniformly found that the Brighton chalybeate, whether it was taken from the well in dry or rainy weather, in ſummer or winter, conſtantly yielded *eight grains and a half* of ſolid reſidue, dried at 160°, for each pint of the water, and the accidental deviations from this proportion, which have occaſionally occurred, have never exceeded a quarter of a grain.

(C)—The proceſs being carried on in the manner juſt related, the whole maſs of ſolid matter preſented itſelf in the form of a greeniſh incruſtation, thick at the bottom, and gradually thinner towards the edges. But on cloſer examination, a quantity of yellow powder, (which afterwards proved to conſiſt chiefly of iron), appeared collected in the centre, and there was ſeen alſo a kind of ſnow-like ſubſtance ſlightly ſpread over the whole ſurface. Viewed through a magnifying glaſs, the whole maſs offered the appearance of a confuſed cryſtallization; and ſoon afterwards ſigns of deliqueſcence manifeſted themſelves by the appearance of ſmall drops of water on different parts of the ſurface. Theſe various appearances deſerved the more notice, as they pointed out the neceſſity, before proceeding any farther, of rendering the reſidue homogeneous, by careful mixture and trituration, in order to obtain correſponding reſults from different portions of the ſame reſidue.

§ VIII. *On the modes of filtration and deſiccation adopted in this analyſis.*

This part of chemical manipulation is of ſuch material importance in an inquiry of this kind, and is ſo intimately connected with the reſults, that although I wiſh to avoid enlarging upon practical details, yet I can hardly proceed farther, without giving once for all, a few explanations which the ſubject ſeems to require.

As the difficulty of getting, in London, large ſupplies of the chalybeate, in its recent ſtate, made it neceſſary to operate upon a ſmall ſcale, it became the more neceſſary to guard againſt the various ſources of inaccuracy to which chemical manipulations are liable. And as it is principally to the different modes of *filtration* and *deſiccation*, that the ſtriking diſcordances which frequently occur amongſt experiments, in other reſpects ſimilarly conducted, are to be aſcribed, it is only on this part of the ſubject that I ſhall offer a few obſervations.

(A)—With regard to filtration, I have, after many comparative trials, adopted the following method. Two round pieces of filtering paper, of between three and four inches in diameter, and exactly ſimilar in weight, are cut from the ſame ſheet, and applied the one over the other, in order to filter the ſolution through both. The reſidue is then, after due deſiccation, weighed, by putting the paper containing it in one ſide of the balance, and the plain piece of paper, ſimilarly dried, in the other. The difference gives the weight deſired. In ſome caſes it may be expedient to ſeparate the reſidue, in order to weigh it by itſelf; but in general the former method is leſs liable to error. A little tedioufneſs in this mode of proceeding no doubt ariſes from filtering through a double paper; but it is ſometimes the only way of avoiding differences, which, however trifling, will, in a complicated analyſis, ariſe from the wetted paper remaining impregnated with various ſoluble ingredients, after the moiſture has been evaporated from it. I attempted alſo to uſe Dr. Black's ingenious method,* which conſiſts in covering every part of the filter with wax, except a ſmall ſpot in the centre, into which the whole of the reſidue ſubſides. But this mode, owing to a variety of little inconveniences, did not appear to me to anſwer the purpoſe ſo well as the method juſt deſcribed.

(B)—With regard to deſiccation, I have uſed an apparatus, which I ſaw firſt in nurſeries, applied to the purpoſe of keeping the food of children of an uniform temperature, and which, with ſome trifling improvements, I have found extremely well adapted to chemical purpoſes. It conſiſts ſimply in a tin pan, about four inches in diameter, and three in depth, in which is placed another ſimilar veſſel, which fits the former pretty exactly, except that it is about half an inch ſhallower, ſo as to form a ſmall ſpace between the two. This ſpace is filled with water, and the ſubſtance to be dried being placed in the upper or ſmaller pan, the heat of a lamp is applied; and in order to prevent oſcillations in the flame, as well as to keep the veſſels ſuſpended over it, the lamp is incloſed in a cylindrical tin caſe, in the ſides of which there are apertures to anſwer the purpoſe of chimnies.

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* See the Analyſis of ſome mineral ſprings in Iceland, in the 3d Volume of the Tranſactions of the Royal Society of Edinburgh.

In placing in the apparatus, the ſubſtance to be dried, care muſt be taken, in order to prevent inaccuracies of temperature, to inſulate that ſubſtance from the apparatus itſelf. For being made of tin,* which is a good conductor of heat, the bottom and ſides of the veſſel are much more quickly heated, than the air or internal ſpace in which the thermometer is immerſed. This is eaſily done by placing the ſubſtance in a thin glaſs capſule, and laying this on a ſmall ſtand conſiſting of three legs of glaſs or fine wire, fixed on a flat piece of cork. Things being thus diſpoſed, and a thermometer being ſuſpended immediately over the capſule, a pretty accurate notion of the temperature to which the ſubſtance is expoſed, will be obtained; and this indication will be ſtill more correct, if the apparatus be covered: but this can only be done towards the end of the proceſs, when moſt of the moiſture is already volatilized.

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* Or *tinned iron*, as it might more properly be called.

(C)—I have only further to obſerve that the temperature which I have generally uſed in deſiccations, is that of 160°; and whenever a different degree of heat has been uſed, it has been expreſsly mentioned. It muſt be confeſſed however, that as it is only by the trimming of the lamp, that the temperature can be regulated, or by putting on, or taking off the cover, occaſional deviations of five or even ten degrees, are ſcarcely avoidable; and the heat, in this apparatus, can hardly be raiſed above 180°. It is in every inſtance to be underſtood, that the ſubſtance has been left expoſed to the ſtated degree of heat, until it has reached its maximum of dryneſs, under that temperature, which will ſometimes take ſeveral hours.

§ IX. *Circumſtances attending the ſolution of the ſolid ingredients of the water.*

Ten grains of reſidue, obtained in the manner deſcribed § VII. 2, being boiled with diſtilled water, the fluid became muddy, and a conſiderable portion of the reſidue remained undiſſolved. A few drops of muriatic acid being then added, and heat applied, the whole was immediately diſſolved, with the exception of a ſmall quantity of ſediment, which, on boiling briſkly, ſeemed for a moment to diſperſe, but ſoon reappeared and ſubſided in the form of a whitiſh powder. I tried to rediſſolve this ſubſtance by means of concentrated acids, and by long boiling and digeſting, but in vain. I therefore began to ſuſpect that the water contained ſome ſiliceous earth, an uncommon occurrence, which, till then, no circumſtance had led me to ſuſpect.

§ X. *Silica.*

(A)—This inſoluble ſubſtance, after being repeatedly waſhed with diſtilled water, and heated to redneſs in a ſmall platina crucible, weighed $\frac{1}{6}$th of a grain. This being mixed with a minute portion of alkali, and tried with the blow pipe, readily melted into a tranſparent glaſs. The experiment being repeated with a different portion of the ſame reſidue, a ſimilar reſult was obtained; only the quantity of ſilica yielded in the latter inſtance, was $\frac{1}{5}$th inſtead of $\frac{1}{6}$th of a grain, a ſmall difference, which gives an average of $\frac{11}{60}$ths of a grain of ignited ſilica, for 10 grains of the ſolid reſidue; or a quantity correſponding to 17 parts of ſilica for 1000 of the ſolid ingredients of the water, dried at 160°. *

(B)—Some ſuſpicion having ariſen that the ſiliceous matter might have been yielded by the glaſs veſſels, and in the proceſſes of ſolution and filtration, in order to aſcertain

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* I ſhall obſerve, once for all, that in giving the reſults of my experiments, moſt of which have been repeated ſeveral times, I have generally, for the ſake of brevity, ſtated only one reſult. Whenever I have been able to trace any particular ſource of error, in any individual experiment, I have repeated it, and have only ſtated the reſult of that which has clearly appeared to be the moſt accurate. But when experiments, conducted exactly in the ſame way, have preſented but very ſlight variations in the reſults, I have, in this caſe, given the average.

this point, a quantity of water, was evaporated in a tin veſſel, and the reſidue of this was treated as in a former experiment. But the ſame reſults were obtained, and no difference could be perceived either in the quantity or nature of this inſoluble vitrifiable matter.

§ XI. *Sulphat of iron.*

Having been led to conclude from the circumſtances before mentioned (§ IV. & V), that a quantity of iron, moſt probably in the ſtate of ſulphat, was contained in the Brighton chalybeate, my next object was to determine in what proportion this ſalt exiſted in the water.

Exper. I.—Twenty grains of the reſidue* were diſſolved in about four ounces of water, by means of a few drops of muriatic acid, and the ſolution was gently heated, a previous ſtep which I have found effectual in facilitating the precipitation of iron by pruſſi-

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[*] When I ſpeak ſimply of *reſidue*, I always mean the reſidue obtained from the Brighton chalybeate, in the manner deſcribed in § VIII. B.

ated alkali; probably in conſequence of its bringing the metal to that ſtate of uniform oxygenation which is moſt favourable to its union with the pruſſic acid. Having then added a ſolution of pruſſiat of potaſh, meaſuring one cubic inch and a half, (a quantity known by previous trials to be ſufficient for the precipitation of the whole iron), the fluid inſtantly paſſed to a blue colour, and a blue precipitate, darker than the ſolution, gradually ſubſided, leaving the ſupernatant fluid tranſparent and nearly colourleſs. After a few hours, the clear fluid was ſeparated by means of a ſyphon, and the remaining muddy fluid, containing the Pruſſian blue, was thrown into a filter. This pruſſiat of iron, after being carefully dried, at the temperature of 160°, weighed exactly 8 grains. I repeated this experiment three times, without any ſenſible variation in the reſult.

Before I could draw any poſitive concluſion from the laſt experiment, with regard to the real quantity of ſulphat of iron, it remained to be determined by comparative trials, what quantity of pruſſiat of iron my ſolution of pruſſiated potaſh, would precipitate from a known quantity of ſulphat of iron. This led me to various inquiries reſpecting the agency of the pruſſic teſt, which are too much connected with the ſubject to be paſſed over in ſilence.

EXPER. II.—Ten grains of green ſulphat of iron, recently prepared and regularly cryſtallized, were diſſolved in water, and after adding a few drops of muriatic acid, the ſolution was gently heated, in order to render this experiment perfectly parallel to the former. One cubic inch and a half of the above mentioned ſolution of pruſſiat of potaſh, being then added, a quantity of Pruſſian blue was inſtantly precipitated, which dried at 160°, weighed 11.3 grains. But in the former experiment 8 grains of Pruſſian blue were obtained from 20 grs. of reſidue. Therefore according to the preſent comparative reſult, it appears that 20 grains of reſidue from the Brighton chalybeate, dried at 160°, contain a quantity of ſulphat of iron equal to 7.079 (11.3: 10=8:7.079) of this ſalt in its cryſtallized ſtate.

Exper. III.—I have hitherto conſidered as a ſtandard of compariſon ſulphat of iron in its *cryſtallized ſtate*, which is not that in which it actually exiſts in the reſidue under examination. In order to aſcertain what allowance ought to be made for this circumſtance, or in other words, how much a given quantity of green ſulphat of iron, in its cryſtallized ſtate, loſt in weight by being deſiccated, 20 grains of the cryſtallized ſalt, reduced to a powder, were expoſed to a heat of 160°, and weighed at different periods of the deſiccation. In a quarter of an hour, the 20 grains were reduced to 17. In about half an hour longer, they were reduced to 15; and after an interval of two hours, which ſeemed to bring the ſalt to its maximum of deſiccation, under that temperature, the 20 grains were reduced to 14; and the ſalt, in this ſtate, was changed to a whitiſh powder.

Exper. IV.—The ſame experiment being repeated, with this difference, that the 20 grains of cryſtallized ſulphat were previouſly diſſolved in water, and then evaporated to dryneſs at the ſame temperature of 160°, the 20 grains were reduced to 12, which is 2 grains leſs than in the preceding experiment.

Taking the laſt of theſe experiments (which ſeems the moſt applicable to the preſent caſe) as a ſtandard of reduction, it will be found that 20 grains of the reſidue contain in fact only 4.24 grains of *dried* ſulphat of iron, a quantity correſponding to the 7.079 grains of *cryſtallized* ſulphat, which were deduced from the former eſtimation. It is obvious therefore, that the mode of calculation juſt propoſed, would be more ſtrictly correct; but the ſtate of cryſtallization being a much more uniform ſtandard of the quantity of moiſture, than any artificial proceſs of deſiccation, I ſhould, on that account, prefer the former mode of computation.

EXPER. V. — The ſolution (Exper. 1) from which the iron had been precipitated by pruſſiat of potaſh, although quite clear at firſt; and having only a greeniſh caſt ſcarcely perceptible, was found, after ſtanding for a few days, to have depoſited an additional quantity of Pruſſian blue, which however was too ſmall to be weighed. Suſpecting from this circumſtance, that ſome minute portion of iron had eſcaped the action of the pruſſiated potaſh, I heated the ſolution with a view to complete the precipitation. But inſtead of the very ſmall additional quantity of precipitate which I expected, the fluid having previouſly paſſed to a muddy green, and then to a blue colour, ſoon depoſited another copious blue precipitate, which, after the uſual filtration and deſiccation, weighed no leſs than 2.8 grains.

Exper. VI.—A ſuſpicion naturally aroſe, that this new pruſſiat, proceeded, at leaſt principally, from the teſt itſelf. In order to aſcertain this point, one cubic inch and a half of the above mentioned ſolution of pruſſiat of potaſh (a quantity equal to that uſed in ex. 1) was boiled, firſt by itſelf, which produced no change or precipitation, and afterwards, with the addition of a few drops of muriatic acid,* which, in a few minutes, occaſioned a copious blue precipitate, weighing, after the uſual filtration and deſiccation, 2.7 grains. It appears therefore that each cubic inch of the teſt, yielded, on being boiled with muriatic acid, 1.8 grains of pruſſiat of iron; and conſequently, that the additional precipitation of 2.8, in the experiment above mentioned, proceeded (with the exception of only $\frac{1}{10}$th of a grain) from the iron contained in the pruſſiat of potaſh, and not from the ſolution under examination.

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* Immediately on adding concentrated muriatic acid to the cold ſolution of pruſſiat of potaſh, a denſe white precipitate appeared, which was inſtantly rediſſolved without any application of heat; but if a conſiderable proportion of acid was added, a permanent white precipitate ſubſided.

As however the quantity of iron thus precipitated from pruſſiat of potaſh, by boiling with muriatic acid, was not the whole of the iron contained in that teſt, I do not entirely depend on the accuracy of this laſt concluſion. For, unleſs the quantities of acid uſed, and the degree of heat applied, be exactly the ſame in both experiments, correſponding reſults cannot be expected. In the preſent inſtance therefore, where theſe circumſtances were not attended to, the coincidence obtained may have been accidental. But it may be remembered, that my eſtimate of the quantity of iron in the chalybeate reſted upon a direct comparative experiment (exper. 2) on artificial ſulphat of iron, the reſult of which was afterwards confirmed by the application of ſuccinat of ammonia (exper. 7 & 8), a teſt totally different from the former. This eſtimate therefore is quite independent of the peculiarities of the pruſſic teſt above mentioned (exper. 5 & 6). But I have thought it right to ſtate them, as they point out the neceſſity, whenever there is an exceſs of acid, of boiling the ſolutions to which the pruſſic teſt has been applied, as a previous ſtep to any other part of the proceſs in which heat may be required; ſince, otherwiſe, a precipitate ariſing from the teſt itſelf, would interfere with the ſubſequent reſults. And it may be alſo obſerved, that, boiling with muriatic acid, provided it be carried to a ſufficient extent, will be, on many occaſions, a convenient mode of ſeparating entirely the iron from ſolutions in which the pruſſic teſt has been concerned; ſince, as Scheele has firſt obſerved, the mineral acids poſſeſs that power.

It may not be uſeleſs to obſerve, that the ſolution of pruſſiated alkali, which I uſed in all theſe experiments, conſiſted of one part of the pruſſiat in cryſtals, to ſixteen parts of diſtilled water; and that a ſpecimen of theſe cryſtals, burnt with a little wax, in a ſilver crucible, yielded 0.225 of brown oxyd of iron. This pruſſiat of potaſh was ſo prepared, as not to be tinged blue by the mineral acids, unleſs heat was applied.

Having thus obtained by means of the pruſſic teſt, reſults which appeared ſufficiently accurate, with regard to the quantity of ſulphat of iron contained in the water, I was deſirous to aſcertain, by ſome other proceſs totally unconnected with this, the real proportion of metallic iron, or rather of oxyd of iron, actually contained in a given quantity of this chalybeate.

For this purpoſe, and in order to obtain a ſolution of this queſtion, perfectly independent of my own experiments, I requeſted of my friend Mr. Allen, one of the lecturers of chemiſtry in the medical ſchool of Guy's Hoſpital, to examine, by any method he might think proper, a portion of reſidue procured in the ſame manner as that which had been the object of the preceding experiments.

He very obligingly complied with my requeſt, and ſoon afterwards favoured me with the following account, which I ſhall give in his own words:

EXPER. VII.—“Ten grains of the precipitate from the chalybeate ſpring at Brighton, dried at the temperature of 160°, were diſſolved in diſtilled water, by the aſſiſtance of a little muriatic acid, with the exception of a minute fraction of a grain, which appears by Dr. Marcet's experiments, to be ſilex. This ſolution was exactly neutralized by ammonia. A ſolution of ſuccinat of ammonia being added, and the whole boiled, a brown precipitate was obtained. This roaſted with wax, in a ſilver crucible, gave 1. 3 grains of oxyd of iron.

“Dr. Marcet having found that the oxyd of iron in this mineral water, is combined with ſulphuric acid, the following comparative experiment was made with the green ſulphat of iron, to determine the quantity of oxyd of iron contained in it, by the teſt of ſuccinat of ammonia.

Exper. VIII.—"Five grains of cryſtallized green ſulphat of iron, were diſſolved in diſtilled water; and the iron precipitated by ſuccinat of ammonia in a boiling heat. The precipitate being treated with wax in a red heat, gave 1.8 oxyd of iron, which was of a reddiſh colour, reſembling cinnamon, but rather darker.

"Then 1.8: 5=1.3: 3.61 grains of green ſulphat of iron, in 10 grains of the precipitate procured by boiling the water down to dryneſs, in a heat not exceeding 160°.

"In employing the ſuccinat of ammonia as a teſt, care muſt be taken to ſaturate the ſolution to which it is applied, very accurately." *

On comparing theſe reſults with my own, I had the ſatisfaction to obſerve that the quan-

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* In addition to this, I would obſerve, that long and repeated boiling, is alſo neceſſary to promote the action of this teſt on ſulphat of iron, upon which it acts but very ſlowly and imperfectly without it. It is evident that the effect of boiling, in this inſtance, depends merely upon a further oxydation of the iron; ſince by allowing the ſolution to ſtand for a ſufficient length of time, or by adding nitric acid, the iron becomes readily and entirely precipitable.

tity of ſulphat of iron, which I concluded to be $\frac{7.079}{20}$ appeared from Mr. Allen's more direct eſtimate, to be $\frac{7.20}{20}$, a degree of coincidence which it can hardly be expected to ſurpaſs in reſearches of this nature.

In order to form an eſtimate of the actual quantity of metal or metallic iron, contained in the chalybeate, I tried the following experiment:

EXPER. IX.—Five grains of iron, filed from the pureſt ſpecimen of malleable iron which I could procure, were diſſolved in diluted ſulphuric acid, by long digeſtion in a gentle heat. This ſolution being previouſly neutralized, was repeatedly boiled and filtered with ſuccinat of ammonia, and afterwards treated with pure ammonia, to precipitate a ſmall remaining portion of iron, which had eſcaped the action of the ſuccinic teſt. The whole of this precipitate being expoſed to a red heat, in an open ſilver crucible, and treated with wax, in the ſame way as in exper. 7 & 8, gave 7.4 grains of a dark red brown oxyd of iron, which was attracted by the magnet. It appears therefore that 100 grains of this oxyd, conſiſted of 67.6 of metal, and 32.4 of oxygen.

By combining theſe reſults with thoſe of the two former experiments, it will be found that the 10 grains of reſidue (which, in exper. 7, yielded 1.3 grains of oxyd of iron), contained really no more than 0.87 grains of metallic iron, or 8.7 in 100 grains. And the actual proportion of metal in 5 grains of cryſtallized ſulphat (which, in exper. 8, yielded 1.8 grs. of oxyd), would be 1.22 grs. or 24.4 in 100; a quantity correſponding to 0.36 of the oxyd.* As however the proportion of oxyd in the ſalt, muſt depend upon its degree of oxydation, theſe reſults, even allowing them to be perfectly accurate, can only be applied to experiments made exactly under the ſame circumſtances as thoſe juſt related. But I ſhould not omit to mention, that my brown oxyd of iron, had a more diſtinct reddiſh hue than that obtained by Mr. Allen, in the experiment to which this is compared;

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* Chemical writers agree in reckoning 0.27 of oxygen in oxyd of iron at its minimum of oxydation, ſuch as it is obtained from pure green ſulphat; and 0.48 in the red oxyd, ſuch as it exiſts in the red ſulphat of iron. In my experiment, the oxyd appeared to contain 0.324, which is a kind of intermediate proportion between thoſe juſt mentioned.

although the proceſs of precipitation and calcination, was, as much as poſſible, carried on in a ſimilar manner. It muſt be obſerved, alſo, that my ſolution of 5 grains of iron, having not been brought to the ſtate of cryſtallized ſulphat, ſome doubts may ariſe as to the degree of oxydation which the metal underwent in that ſolution.

Before I conclude my obſervations on this part of the ſubject, I ſhould not omit to remark, that wherever in the courſe of this inquiry a quantity of iron has been precipitated from the water, whether merely in conſequence of its being kept for ſome time (as in exper. 2. D), or by means of concentration, it has never preſented itſelf in the ſtate of ochre, or ſimple oxyd of iron; but on the contrary, has always, upon careful examination, appeared to be combined with a portion of ſulphuric acid, forming what has been called a ſub-ſulphat of iron.

§ XII. *Red ſulphat of iron.*

EXPER. I.—Twenty grains of reſidue were put in a phial, with about 150 grains of alcohol, of the ſpecific gravity of 8.10, and the mixture, after being often ſhaken and kept well corked for a few hours, was thrown into a filter. This filtered ſpirituous ſolution was of a reddiſh yellow colour. Being evaporated to dryneſs, it depoſited a deliqueſcent browniſh reſidue, which being rediſſolved in water, imparted to it a deep yellow colour. But a ſmall portion of this reſidue (which proved to be a ſub-ſulphat of iron, formed by the action of the atmoſphere, during the proceſs of evaporation) remained undiſſolved, till a little muriatic acid was added. The reſidue which had been depoſited in the filter by a ſpirituous ſolution, was become of a paler colour, in conſequence of its being treated with alcohol, and had loſt its deliqueſcent quality. Its weight was reduced to 14.1 grains.*

Exper. II.—As it is well known that alcohol has the property of diſſolving red ſulphat of iron, whilſt on the contrary it precipitates the green ſulphat, there could be no doubt but that the deliqueſcent reſidue, obtained in the former experiment, contained a quantity of red ſulphat of iron. In order to learn in what proportion this ſalt was contained in a given quantity of the reſidue, the yellow watery ſolution above mentioned, proceeding (exper. 1) from 20 grains of reſidue, was treated with pruſſiat of potaſh, which occaſioned a precipitation of Pruſſian blue, weighing 1.45; which denotes a quantity of red ſulphat of iron, equivalent to 1.3 grains of the green ſulphat.

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* Yet the reſidue of the ſolution in alcohol dried at 160°, weighed only 3.1, inſtead of 6.9 grains, which would have been the complement of the 20 grains of reſiduous matter. This muſt be owing to a loſs of moiſture, in conſequence of the action of alcohol.

(A)—But although the preſence of red ſulphat in the reſiduary matter, is manifeſtly ſhewn by theſe experiments, yet they do not by any means prove, that the ſalt actually exiſted in the recent chalybeate. On the contrary it appears probable, that it is the product* of the ſeveral operations to which the water has been ſubmitted. This I think may be inferred, not only from the tincture of galls not tinging the recent chalybeate black, but alſo from the conſtant formation and precipitation of ſub-ſulphat, which happens whenever the water is expoſed to a proceſs of oxygenation. For it is reaſonable to ſuppoſe, that, whilſt on one hand, a proceſs of oxygenation, and conſequent ſeparation of oxyd of iron from the acid which held it in ſolution, takes place, the portion of acid thus liberated, will unite with as much of the ſuperoxygenated oxyd as it will be capable of combining with, and thus a quantity of the red or ſuperoxygenated ſulphat will be generated. This queſtion however, is, in the preſent inſtance, a mere matter of curioſity, ſince it cannot affect in any material manner the former reſults obtained, either as to the abſolute quantity of iron in the chalybeate, or as to the nature of its combination.

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* Mr. Kirwan, in his "Eſſay on the Analyſis of Mineral Waters," in which he has ſo much contributed to the advancement of that part of chemical ſcience, expreſſes a belief that the ſulphat of iron, which has occaſionally been diſcovered in mineral waters, did not exiſt in thoſe waters, previous to the analytic proceſſes to which they were ſubjected.

§ XIII. *Muriat of iron.*

I have ſuggeſted in a former part of this paper, the poſſibility of ſome muriat of iron being contained in the water. This conjecture, the experiments above related, do not preciſely contradict, but they render it extremely improbable.

In the firſt place it muſt be remembered, that if any muriated iron did exiſt in the water, it could only be in a very minute quantity, ſince it muſt have made part of the ſmall portion of red ſulphat of iron which was eſtimated from the preceding experiments, both theſe ſalts being equally ſoluble in alcohol.

Had the ſpirituous ſolution contained no other ſalts but the red ſulphat or muriat of iron, the preſence or abſence of the latter would have been eaſily demonſtrated by nitrat of ſilver, or any other teſt of muriatic acid. But in this caſe, as it will be proved hereafter, other muriats were diſcovered, which prevented my obtaining an abſolute proof of the water being perfectly free from the preſence of muriated iron.

§ XIV. *Sulphat of lime.*

Exper. I.—Oxalat of ammonia being added to a ſolution of 10 grains of reſidue, (the iron of which had been previouſly ſeparated by ſuccinat of ammonia), a conſiderable turbidneſs inſtantly took place, and a white precipitate readily ſubſided, which, dried as uſual, weighed 4.1 grains.

Having had every reaſon to conclude from ſome of the preliminary experiments, mentioned in a former part of this paper, that the lime exiſted in the water in the ſtate of ſulphat, it remained to be determined, what quantity of ſelenite correſponded to the 4.1 grains of oxalat of lime, obtained from 10 grains of reſidue.

Not finding in chemical works any facts from which I could immediately deduce the ſolution of this queſtion, I made the following comparative experiment:

Exper. II.—A ſpecimen of the pureſt native ſelenite that I could procure, was pulverized and boiled to ſaturation in diſtilled water. This ſolution was decanted, and the clear fluid evaporated to dryneſs in the temperature of 160°. Five grains of the ſelenite thus obtained and dried, being rediſſolved in water, and afterwards precipitated by oxalat of ammonia, and dried at 160°, weighed 4.25 grains; which is equivalent to 117 parts of ſelenite, for 100 parts of oxalat of ammonia. Conſequently, 4.82 grains (4.25 : 5=4.1 : 4.82) will be the quantity of ſelenite contained in 10 grains of reſidue, dried at 160°*

§ XV. *Alum and Epſom ſalt.*

The preſence of theſe two earthy ſulphats in the water, being by no means improbable, I tried to diſcover them in the following manner.

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* This eſtimate of courſe ſuppoſes that ſelenite is the only calcareous ſalt preſent in the water. Muriat of lime, which is often found in mineral waters, could not be expected in this, ſince its exiſtence, except in extremely minute quantities, is incompatible with ſulphat of iron. In confirmation of this, I tried the muriatic ſalts obtained from the ſolution in alcohol (§ XIII. exp. 1), with oxalat of ammonia, which occaſioned only a very ſlight cloud, without any ponderable precipitate.

Exper. I.—The portion of reſidue which had been left in the filter (§ XII. exper. 1), by the ſolution in alcohol, was diſſolved in a quantity of diſtilled water, not ſufficient to take up any conſiderable quantity of ſelenite, and the filtered ſolution was evaporated in a temperature not exceeding 80°. As the evaporation advanced, regular cubic cryſtals depoſited themſelves on the bottom of the veſſel; and when it was completed, there appeared alſo, beſides the cryſtals (which formed the greateſt part of this reſidue), a minute quantity of a yellowiſh powder, collected in the centre of the cup, and a kind of ſnow-like efflореſcence ſlightly ſpread over it. But no other cryſtals but thoſe juſt mentioned could be diſcovered. The whole of this reſiduary maſs, dried as uſual, weighed preciſely 5 grains. But the heat applied in the proceſs of drying, was not ſufficient to deprive the cryſtals of their water of cryſtallization. From the form of theſe cryſtals, from their decrepitation on being heated, and their well known taſte, there could be no doubt but that they were muriat of ſoda.

Exper. II.—In order to obtain more poſitive information in this reſpect, and alſo to aſcertain the nature of the ſmall portion of uncryſtallized matter mixed with the cryſtals, the reſidue of 5 grains above mentioned, was mixed with about 2 ounces of diſtilled water, which rediſſolved the whole of it, with the exception of a very minute quantity of a yellowiſh powder. The following teſts were then tried:

(a) Pruſſiat of iron produced a pale green colour, and a precipitate weighing $\frac{6}{10}$ths of a grain.

(b) Oxalat of ammonia occaſioned a precipitate weighing 1.6 grains.

(c) Nitrat of ſilver, and muriat of barytes, both produced copious precipitates.

(d) A ſolution of platina in nitro-muriatic acid, produced no precipitate whatever.*

(e) The ſmall quantity of undiſſolved

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* This teſt is exceedingly convenient and deciſive with regard to the preſence of potaſh, as it forms a diſtinct and immediate precipitate with the ſmalleſt portion of this alkali, or any of its compounds, whilſt it is not at all affected by the mineral alkali.

yellow powder, which weighed at moſt $\frac{1}{10}$th of a grain, proved to be ſubſulphat of iron.[*]

(*f*) After the complete removal of the iron and lime, the remaining ſolution was tried both with pure ammonia and pure potaſh, which occaſioned no cloud or precipitate whatever; whilſt the leaſt quantity of any earthy ſalt, added to this ſolution, was readily diſcovered by the alkali.

From theſe experiments I inferred, that the 5 grains ſoluble in water conſiſted of about 2 grains of ſulphat of iron and ſelenite, and 3 grains of muriat of ſoda: and from the laſt reſult in particular, I concluded, that neither alum, nor ſulphat of magneſia, were contained in the water.

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[*] It may be obſerved that the quantity of ſulphat of iron yet diſcovered, either in the ſpirituous or the watery ſolution, does not amount to more than 2 grains, inſtead of the 7 grains which the 20 grains of reſidue under examination ſhould contain, according to my former ſtatements. But the remaining 5 grains were found in the ſtate of ſubſulphat, in the reſidue inſoluble both in alcohol and water, which conſiſted entirely of ſelenite and ſubſulphat of iron.

§ XVI. *Muriats of Alumine and Magneſia; and an inquiry into the properties of ſuccinat of ammonia as a teſt of alumine.*

EXPER. I.—Twenty grains of reſidue were rediſſolved in dilute muriatic acid, and boiled with pruſſiat of potaſh, till the iron was completely removed. The ſolution was afterwards neutralized, and the lime ſeparated by oxalat of ammonia. A ſolution of pure potaſh being then added, in order to precipitate the alumine and magneſia, whitiſh flakes gradually ſubſided, which being collected on a filter, and dried at 160°, weighed 2.1 grains.

EXPER. II.—This precipitate was boiled in a ſolution of pure potaſh, with a view to diſſolve the alumine, and thus ſeparate it from the magneſia, which is not ſoluble in alkali. This ſolution being allowed to cool, ſoon depoſited a whitiſh powder, and the fluid, which continued ſomewhat turbid, being decanted off, the white powder, dried as uſual, was found to have loſt about half of its weight.

From theſe firſt reſults I naturally conjectured, that the 2.1 grains conſiſted of nearly equal parts of alumine and magneſia. This however, as it will ſoon appear, was contradicted by ſubſequent experiments.

Exper. III.—The whitiſh powder inſoluble in potaſh being examined, proved to be magneſia, mixed with a ſmall quantity of lime, which was readily diſcovered by the oxalic teſt. But the ſuppoſed ſolution of alumine in potaſh, being allowed to ſtand for ſome time, depoſited a further quantity of a whitiſh powder, and the clear alkaline liquor being boiled with muriat of ammonia, produced no precipitation whatever, which led me to ſuſpect that the whole of the original precipitate (exper. 1) was magneſia, with the exception of the minute quantity of lime juſt mentioned.

Exper. IV.—In hopes to obtain more deciſive reſults on this ſubject, and in order to vary the laſt experiments, I made a new ſolution of 10 grains of reſidue, from which the iron was precipitated by ſuccinat of ammonia. But from this ſolution, treated like the former with cauſtic alkali, I could obtain no earthy precipitate.

Exper. V.—This circumſtance, as it appeared afterwards, was ſimply owing to the ſtate of very great dilution in which the earthy ſalt exiſted in this ſolution; but before I was aware of this, an idea occurred to me, that the non-appearance of a precipitate in this caſe, might poſſibly ariſe from the previous application of ſuccinat of ammonia. In order to aſcertain this point, I prepared artificial ſolutions of muriat of alumine, and muriat of magneſia, and obſerved, that on pouring a few drops of ſuccinat of ammonia into the former, a copious precipitate ſubſided.

It appeared therefore uſeleſs to proſecute any farther my inquiry on the above ſolution. But this property of ſuccinat of ammonia, to precipitate alumine, ſtruck me as being new and curious, and attracted for ſome time my attention.

(A)—I ſhall not enter here into all the particulars of the inquiry and various experiments reſpecting alumine and its relations with the ſuccinic teſt, which aroſe from the circumſtance juſt related; but as the ſubject is by no means unconnected with the object of this eſſay, and as ſome explanation may be of uſe to facilitate the application of this teſt to the examination of ſolutions of alumine, I ſhall mention the following general reſults:

1ſt, That ſuccinat of ammonia precipitates alumine from its combinations, readily and entirely, provided there be no conſiderable exceſs of acid, in which caſe the ſolution muſt be previouſly neutralized.

2dly, That this ſubſtance does not appear to have any action on magneſia or its compounds, and will therefore in many caſes afford a new and convenient diſcriminating teſt between this earth and alumine, the method of boiling the alumine in potaſh being in ſome inſtances inapplicable. *

3dly, That if a ſolution of 100 parts of

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* Since this was written, I have ſeen in the *Journal des Mines*, N° 70, a paper on Yttria, by Mr. Ekeberg, in which the author notices the property of ſuccinat of ammonia to precipitate glucine, which is another common feature between this earth and alumine. But I do not find that either Mr. Ekeberg or any other writer has noticed the property of this teſt with regard to alumine. On the contrary I obſerve, that the celebrated chemiſt Klaproth (Analytical Eſſays, vol. ii), in his analyſis of the Gadolinite, a mineral which contains a ſmall quantity of alumine, precipitated the iron by ſuccinat of ammonia, without remarking the property alluded to, although it could not fail to have ſome influence on the reſult.

octahedral cryſtals of alum be decompoſed by ſuccinat of ammonia, the precipitate, calcined in a red heat, weighs exactly 12. This reſult I offer with the more confidence, as it agrees perfectly with Mr. Kirwan's ſtatement, a coincidence which I did not notice until I had completed my own experiments.

4thly, That if a ſimilar ſolution be decompoſed by pure ammonia, the precipitate, calcined in the ſame manner, gives preciſely the ſame weight.

5thly, That however there is this difference between the precipitates produced from alum by ſuccinat of ammonia, and thoſe obtained from pure ammonia, that in the firſt inſtance, the precipitate, dried at 160°, is white like ſtarch, and weighs 35; whilſt in the latter, the precipitate, dried in the ſame manner, ſhrinks to a browniſh powder, ſomewhat reſembling glue coarſely pulverized, and weighs only 15, inſtead of 35.*

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* It has been ſhewn by my ingenious friend Mr. Theodore De Sauſſure, in his valuable paper on alumine (publiſhed in the *Journal de Phyſique*, vol. 52), that this peculiar colour and ſhrinking of alumine, depends on the great proportion of water in the ſolution from which it is precipitated; and that this appearance is entirely prevented, by uſing a concentrated ſolution of alum.

6thly, That if 100 parts of pure alumine prepared from alum, and brought to the conſiſtence of a paſte,* be diſſolved in ſulphuric acid, and afterwards precipitated by ſuccinat of ammonia, and calcined in a red heat, the reſidue will be exactly equal in weight, to that procured by calcining in the ſame manner, without any previous ſolution and precipitation, a ſimilar quantity of pure alumine taken from the ſame maſs.

Exper. VI.—Being now poſſeſſed of a direct mode of precipitating alumine, I made a ſolution of 10 grains of reſidue, and after precipitating the iron from it by pruſſiat of potaſh, and the lime by oxalat of ammonia, I added ſuccinat of ammonia. But not the leaſt effect was produced, although cauſtic alkali occaſioned a precipitate, and although the leaſt quantity of muriat of alumine produced an immediate cloudineſs. This experiment, which I repeated ſeveral times with the ſame reſult, appearing ſufficiently deciſive, with regard to the abſence of alumine in the Brighton chalybeate,* my attention was now excluſively directed to the magneſia, the preſence of which had been proved by former experiments.

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* I procured this paſte by precipitating a ſolution of alum by pure ammonia, and afterwards waſhing the precipitate repeatedly in great quantities of diſtilled water, and heating it gently once or twice with ammonia. The water at laſt came off perfectly free from ſulphuric acid; and a portion of the paſte thus prepared, being diſſolved in muriatic acid, and a ſtrong ſolution of muriated barytes added, a ſlight cloud only was produced, without any diſtinct precipitate. And as nitrat of barytes produced no cloud whatever, I ſuſpected that the ſlight effect of muriated barytes might poſſibly be one of thoſe anomalies, ſuch as Mr. Kirwan has obſerved with regard to muriat of magneſia, quite independent of the preſence of any foreign ſubſtance.

EXPER. VII.—Magneſia, in this inſtance, as it has been ſhewn before (§ XV. exper. 2. f), could only be ſuppoſed to exiſt in the state of muriat. In attempting to eſtimate the quantity of this ſalt, I had again recourſe to alcohol. 20 grains of reſidue were put into a phial, with about 120 grains of alcohol of the ſpecific gravity of 810. The undiſſolved part was allowed to ſubſide, after ſhaking the ſolution repeatedly, and letting it ſtand for ſeveral hours. The clear fluid being then decanted off, and a ſolution of carbonat of ammonia *fully ſaturated* with carbonic acid, being added, the mixture became thick and turbid, and paſſed to a dirty brown colour. This muddy fluid being filtered, and phoſphat of ſoda added to the clear ſolution, a cloudineſs immediately appeared, and in a few minutes a white powder ſubſided, which, when dried at a temperature not exceeding 100°, weighed 2.8 grains.

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* Whilſt this ſheet was printing, I accidentally obſerved, that muriat of alumine was decompoſed by the pruſſiat of potaſh which I uſed in my experiments, and which, I have every reaſon to ſuppoſe, had been prepared with ſufficient care and accuracy. I have ſince found upon inquiry, that this property, though not generally known, has been noticed by ſome chemiſts, and probably belongs to all pruſſiated alkalies, in whatever manner they have been prepared. No concluſion therefore, reſpecting the non-existence of muriat of alumine, can be deduced from the above experiment, nor from any other proceſs in which the pruſſic teſt has previouſly been uſed. But from many other circumſtances, and particularly from the reſults obtained in the analyſis by alcohol, it appears ſufficiently obvious, that muriat of alumine (unleſs it be in extremely ſmall quantity), cannot exiſt in the water.

The precipitate obtained by this method*, being a triple ſalt, compoſed of phoſphoric acid, magneſia and volatile alkali, (the ſame combination which Mr. Fourcroy has diſcovered in the bladder of a horſe, and Dr. Wollaſton has ſhewn to compoſe one of the concretions which are formed in the human bladder), my next object was to determine, by a comparative experiment, what quantity of muriat of magneſia was required to form, by the proceſs above mentioned, a known quantity of this triple ſalt.

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* This very eaſy and valuable method of precipitating magneſia, was firſt ſuggeſted by Dr. Wollaſton. It is obviouſly founded upon the property which fully neutraliſed carbonat of ammonia poſſeſſes, firſt to diſſolve the carbonat of magneſia which is formed in conſequence of a double elective attraction, and afterwards to yield the earth to the phoſphoric acid, with which, and the ammonia, it forms a triple ſalt. With

EXPER. VIII.—For this purpoſe, I evaporated to dryneſs a ſolution of muriat of magneſia, in a heat of 160°. 5 grains of the reſidue thus obtained were diſſolved in water, and both

regard to the preparation of the carbonat of ammonia, Dr. Wollaſton's method conſiſts ſimply in pulveriſing a quantity of the common carbonat, and expoſing it for a few hours to the action of the atmoſphere, thinly ſpread on a piece of paper. I found that 100 grains of common carbonat of ammonia recently ſublimed, being treated in this manner, were reduced to 57 grains: and a ſolution of ſalt thus prepared had no ſmell whatever. But no certain inference can be drawn from this experiment, reſpecting the proportions of carbonic acid and volatile alkali in the common carbonat, ſince (as Mr. Davy has ſhewn in his excellent treatiſe on the nitrous oxyd), thoſe proportions vary conſiderably according to the degree of heat with which this ſalt is ſublimed.

carbonat of ammonia, and phoſphat of ſoda were ſucceſſively added, as in the former experiment. The precipitate, dried in a heat not exceeding 100°, weighed 7.8 grains; and the ſame was reduced to 7 grains, by raiſing the heat to 120°. It appeared in the form of an impalpable, and nearly taſteleſs white powder.

This reſult therefore, combined with that of the former experiment, gives 1.79 grains (78 : 5 = 28 : 179), as the quantity of muriat of magneſia contained in the 20 grains of reſidue.

EXPER. IX.—In order to know what quantity of pure magneſia the above 2.8 grains of triple ſalt contained, I made the following comparative experiment: *

Five grains of magneſia, prepared by ex-

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* It may be obſerved, that in the comparative experiments, I have generally uſed but ſmall quantities of the ſubſtance to be examined, though it would have been very eaſy to employ larger quantities of materials. But it appears to me, that generally ſpeaking, there is ſeldom any thing to be gained by making experiments of inquiry upon a large ſcale; whilſt on the contrary, ſmall quantities are, with proper management, more ſuſceptible of giving expeditious as well as accurate reſults, particularly when the proceſſes of filtering and drying are concerned.

poſing for about a quarter of an hour, common calcined magneſia to a red heat, ſo as to be certain that all the moiſture and the carbonic acid had been expelled, were diſſolved in muriatic acid, and precipitated as in exper. 7. The triple ſalt being collected on a filter, and thoroughly dried in a heat not exceeding 100°, weighed 26.3 grains. Conſequently 2.8 grains of this ſalt, or 1.79 grains of muriat of magneſia, contained only 0.53 grains of pure magneſia; a proportion which is equivalent to 100 grains of triple ſalt dried at 100°, for 19 of magneſia prepared in the manner juſt deſcribed. I dried the 26.3 grains of triple ſalt at a temperature not exceeding 100°, fearing that it might be decompoſed by a greater heat; but I was rather ſurpriſed to find, that being afterwards expoſed to a heat of 160°, it loſt only 2 grains.

§ XVII. *Sulphuric acid.*

In order to aſcertain the quantity of ſulphuric acid in the chalybeate, a ſolution of nitrat of barytes was added to a pint of the water (equivalent to $8\frac{1}{2}$ grains of ſolid reſidue), which precipitated from it 6.8 grains of ſulphat of barytes*; a quantity which, ſuppoſing this ſalt to contain 0.33 of acid, according to the aſſertion of Fourcroy and Kirwan, would correſpond to 2.24 grains of ſulphuric acid in a pint, or 2.64 in 10 grains of the reſidue.

§ XVIII. *Muriatic acid and muriat of ſoda.*

Exper. I.—Ten grains of reſidue were diſſolved in very dilute nitric acid, and nitrat of ſilver was added, in order to ſeparate the muriatic acid. The precipitate dried as uſual, weighed preciſely 6 grains; a quantity which, (if according to Mr. Kirwan,† 100 parts of muriated ſilver contain 16 parts of acid) would make 0.96 grains of muriatic acid in 10 grains of reſidue.

(A) With regard to the quantity of muriat of ſoda, it will be found that if according to Dr. Black's ſtatement*, 235 grains of luna cornea are equivalent to 100 grains of common ſalt, the above 6 grains of muriated ſilver will repreſent 2.5 grains of muriat of ſoda: and by deducting from theſe 2.5 grains, 0.7 grains (which may be conſidered as an adequate allowance for the 0.89 grains of muriat of magneſia diſcovered in XVI. exper. 7 & 8), the quantity of muriat of ſoda will be reduced to 1.8 grains, in 10 grains of reſidue.

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* Ten grains of reſidue would therefore have yielded 8 grains of ſulphated barytes dryed at 160°.

† See Kirwan on Mineral Waters, table IV. Mr. Kirwan's preciſe eſtimate is $\frac{16.54}{100}$ of acid, the ſalt being dried at 130°.

§ XIX. *Examination of the ſediment depoſited by the chalybeate, and of the changes which the water undergoes by long expoſure to the atmoſphere.*

(A) I have mentioned at the beginning of this paper a copious earthy or ochry ſediment which is formed wherever the water has been ſuffered to ſtagnate. Some of this ſubſtance

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[*] See Tranſactions of the Royal Society of Edinburgh III. 116.

which I had collected and brought with me in a muddy ſtate, became in a few weeks hard and friable like ochre, of which it had exactly the colour and appearance. Being ſlightly examined, it appeared to contain iron, in the ſtate (partly at leaſt) of ſubſulphat; lime, chiefly in the ſtate of carbonat; and a ſmall portion of argillaceous earth. The two laſt ingredients, there is reaſon to ſuppoſe, are merely conveyed and depoſited by the water, without having ever been chemically combined with it.

(B) With regard to the pond before mentioned, formed by the ſuperfluous water from the Wick chalybeate, I found on examination, that a pint of it contained only 3½ grains of ſolid matter, in which I diſcovered nearly the ſame ingredients as in the freſh chalybeate, except that the whole of the iron had diſappeared. The nature of this water, however, muſt vary exceedingly according to the weather and the ſeaſon; ſince heavy rains muſt always dilute its contents, and ſometimes occaſion the introduction of various other ingredients.

§ XX. *Recapitulation, and concluſion.*

On recapitulating the various reſults of this analyſis, in order to preſent them in one view, it appears, that 100 parts of the ſolid reſidue obtained by evaporation from the Brighton chalybeate, and dried at the temperature of 160°, have given:

	GRAINS.		
Sulphat of iron	21.2	equal to 35.3 grs. of cryſtallized green ſulphat.	Dried at 160°
Sulphat of lime	48.2		
Muriat of ſoda	18.		
Muriat of Magneſia	8.9		
Siliceous earth	1.7		
Loſs	2.0		
	100		

And a pint of the water (which is equal to 8.5 grains of the ſolid reſidue) contains:

Carbonic acid gas, about $2\frac{1}{2}$ cubic inches, or $\frac{1}{13}$th part of its volume:

	GRAINS		
Sulphat of iron -	1.80	... equal to 3 grains of cryſtallized green ſulphat.	Dried at 160°
Sulphat of lime -	4.09		
Muriat of ſoda -	1.53		
Muriat of magneſia	75		
Siliceous earth -	- 14		
Loſs - - - - -	- 19		
	8.50.		

I ſhall not prolong this paper by remarks on the application of the Brighton chalybeate to the cure of diſeaſes, as I have no perſonal experience of its medicinal properties. But I may be allowed to point out the conſiderable proportion of active ingredients, and more particularly of iron, which it contains, compared to other ſprings of an analogous compoſition. The chalybeate of Tunbridge, for inſtance, which has long been juſtly celebrated for its medicinal virtues, contains, according to Dr. Babington's analyſis, no more than 1 grain of oxyd of iron in a gallon; whilſt the Brighton ſpring holds in one pint, more than an equal quantity of the ſame oxyd. It is true that the chemical compoſition of the two ſprings is very different, ſince in the one, the iron is ſuſpended by a fixed acid, whilſt in the other, it is held in ſolution by a gaſeous menſtruum. But experience has ſhewn that the ſpecific effects of iron on the human frame, do not materially differ, whether it be taken in the ſtate of carbonat, or of ſulphat, or of ſimple oxyd, or in any other form, provided the metal be nearly in the ſame ſtate of oxydation; although it muſt be allowed that there may be differences in the degree of that effect, as well as in the manner in which the acid, or vehicle of the metal, affects the organs of digeſtion, in particular individuals. It may be obſerved alſo, that this ſpring contains a quantity of neutral ſalts, which may probably be of uſe in affording a gentle ſtimulus to the ſtomach and inteſtines, and thus contributing to inſure the beneficial effects of its ferruginous ingredients. It is therefore very reaſonable to ſuppoſe, that the Brighton chalybeate is likely to be of conſiderable uſe in diſeaſes which require the tonic powers of this kind of medicines; and from the peculiarly advantageous ſituation of that ſpring, there is every reaſon to expect, that at no diſtant period, its medicinal qualities will be more generally acknowledged.*

Before I conclude, I muſt beg leave to mention a circumſtance reſpecting the medicinal effect of this chalybeate, which I have learnt from Dr. Tierney. This gentleman has obſerved, that in common with moſt mineral waters of this kind, it is apt to occaſion in ſome individuals a degree of nauſea and a ſenſe of weight in the ſtomach, when taken cold; but he has found that thoſe effects generally diſappear, if the water be drank moderately warm. This remark appears to me important, and is particularly applicable to the Brighton chalybeate, as its analyſis ſhews, that no iron is precipitated, nor any other material change produced, by applying heat to it, provided it be done quickly, and in veſſels which, from their ſhape, expoſe but a ſmall ſurface of the water to the action of the atmoſphere. In the Tunbridge chalybeate, on the contrary, ſuch a method would be quite impracticable, ſince that water cannot be heated, without the oxyd being immediately precipitated.

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* Several practitioners at Brighton, have been for ſome years in the habit of recommending the Wick water in a variety of complaints. Both Dr. Tierney and Mr. Hall have told me, that they have had frequent opportunities of aſcertaining its beneficial effects. That ſpring, however, is yet comparatively ſpeaking, but little known, and ſeldom reſorted to.

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